Q
11
N524

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

SER. II, VOL. 8 JUNE, 1946 No. 8

SECTION OF GEOLOGY AND MINERALOGY

MAY 6, 1946

DOCTOR GEORGE GAYLORD SIMPSON, Chairman, Department of Geology and Paleontology, The American Museum of Natural History; and Professor of Vertebrate Paleontology, Columbia University, New York, N. Y.: *Tertiary Land Bridges.* (This lecture was illustrated by lantern slides.)

An early discovery of paleontology was that animals now confined to one part of the globe may occur as fossils in different and distant regions. In some cases, such as that of the elephants and their allies, it would now be impossible for the animals to migrate between the regions where they currently occur, and regions where they were formerly abundant. It was necessary to conclude that regions now separated by the oceans have been connected by land, at various times in the geological past. It has also been found that continents now connected by land must have been separated during some geological epochs.

The problems involved in these varying land connections and separations are among the borderland studies that overlap two, or more, scientific disciplines. Solutions of these problems are essential to both geologists and zoologists, and such solutions can only be reached by combined study, from both the geological and the zoological point of view. Some zoologists have concluded that the recognition of related, or supposedly related, animals in two widely separated regions justifies the postulation of a direct land route between those regions, even though this may be in reckless violation of geological principles and

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 8, No. 8, June, 1946.
This publication is distributed to Members and is published monthly from November to June, inclusive, at 109 West Chestnut Street, Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.
Editor: Roy Waldo Miner.
Assistant Editors: Michael Demarest, Lothar N. Salin.
Executive Secretary: Eunice Thomas Miner.
Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

probabilities. Some geologists, particularly a few of the advocates of continental drift, have postulated continental connections of a sort and at times and places indicated as practically impossible by the zoological data.

There have also been numerous sober and careful studies of intercontinental connections, or their absence, but even these have necessarily been incomplete and inconclusive. My efforts to evaluate these studies, and to contribute to making them more precise, have been based primarily on the re-examination of faunal relationships that are known, beyond reasonable doubt, to have involved the rise and fall of intercontinental land bridges. The simplest and clearest case available is the relationship between North and South American land faunas during the Cenozoic. This has been analyzed elsewhere, in some detail.[1] Among other things, there emerge, from this and similar studies, certain faunal criteria for the existence, position, and nature of land bridges, especially that postulation of a given bridge requires evidence of:

1. Exchange of varied types of land life, not only of one kind of animal.

2. Faunal interchange in both directions.

3. Interchange involving or maintaining ecologically balanced faunas.

When these conditions are not met by a postulated land bridge, the alternatives to be considered are:

1. Indirect migration, *e.g.*, *via* the well-established North American-South American and North American-Eurasian bridges, rather than by a direct trans-Pacific Asian- (or Australian-) South American bridge.

2. Migration without a continuous bridge, by what I have called a sweepstakes route, *e. g.*, between Eurasia and Australia, or the American continents and the West Indies.

More recently, I have been working on the application and extension of these and other principles, with regard to Eurasian-North American Cenozoic faunal and continental relationships, a much more com-

[1] See, especially, **Simpson, G. G.**, Mammals and land bridges. J. Washington Acad. Sci. **30**: 137–163. 1940; and **Simpson, G. G.**, Turtles and the origin of the fauna of Latin America. Am. J. Sci. **241**: 413–429. 1943.

Cyclo
New York Academy of Science
8-I-46

plex problem than that of South American-North American relationships. There is a very large literature on this subject, but the best general reviews of it are now far out of date,[2] and much of the literature is fragmentary, excessively subjective, or otherwise inadequate. There are two main lines of evidence, which are frequently confused: first, earliest dates of common appearance of given groups on both continents, which are limiting dates for the migrations of these groups; and, second, fluctuations in faunal resemblance of the continents. In each case, not only the overall statistical picture, involving numbers of migrant groups and degrees of resemblance, but also the nature of the animals concerned, especially their ecological and climatic preferences, are pertinent.

An attempt has been made to place this study on a more objective and quantitative basis than heretofore. There are various difficulties and shortcomings in the quantitative approach. These have to be studied carefully, and applied with due caution, but preliminary results are encouraging. The work is still under way, and it would be premature, now, to give any detailed conclusions. Some of the preliminary results, however, seem likely to stand up.

As regards frequency of migration between Eurasia and North America, preliminary study indicates an early Eocene climax, a mid-Eocene low, a dual late Eocene-early Oligocene climax, a middle-to-late Oligocene low, slow resumption in the early Miocene, building steadily to a dual late Miocene-early Pliocene climax, another dual late Pliocene-Pleistocene climax, and then a drop to the Recent low. Migration is only one of at least five separable factors involved in faunal resemblance. Faunal resemblance does not vary directly with the frequency of intermigration, and its measurement and analysis are particularly complex and difficult, although reasonably good results have been obtained and better are in sight.

The presence, or absence, of a land bridge and, when present, its position, do not follow as directly from the evidence as to frequency of migration and degree of faunal resemblance as has commonly been supposed. As far as this new analysis of the faunal data has yet been followed, it suggests that there was a Eurasian-North American land bridge almost continuously throughout the Cenozoic, with probable

[2] Notably: **Osborn, H. F.**, The Age of Mammals. Macmillan. New York. 1910.

interruptions of appreciable duration only in the middle Eocene, middle to upper Oligocene, and Recent. As regards the position of the bridge, or bridges, it is a surprising result of this study that no good evidence is found for any Atlantic, direct European-North American, bridge during the Cenozoic, contrary to my own former opinion and that of most paleogeographers. The evidence throughout is consistent with a single Siberian-Alaskan bridge, as adequate to explain all the known facts.

SECTION OF BIOLOGY

MAY 13, 1946

Symposium on *"The Place of the Standard Dog in Medical Research."* The program consisted of the following papers*:

"The Physiological and Biochemical Standardization of the Dog," by James B. Allison, Bureau of Biochemical Research, Rutgers University, New Brunswick, N. J.

"Phases of Conditioned Reflexes of Dogs used in Medical Research," by Hans Molitor, Merck Institute for Therapeutic Research, Rahway, N. J.

"Some Practical Aspects of Selecting and Maintaining Dogs for Medical Research," by Mark L. Morris, New Brunswick, N. J.

* The papers by Doctor Allison and Doctor Morris are published in this number of the Transactions. No abstract of the paper by Doctor Molitor has been received.

Doctor James B. Allison, Bureau of Biological Research, Rutgers University, New Brunswick, N. J.: *The Physiological and Biochemical Standardization of the Dog.**

A scientist never uses a delicate apparatus in the laboratory without carefully checking and calibrating it. Too often, however, an animal, such as the dog, is used as an instrument of measurement without much knowledge of its physiological state. There is no more delicate, nor complex machine than the living system, a machine that requires much thoughtfulness and knowledge to use properly in the laboratory. This machine, that we call living, can be described, grossly, as a dynamic equilibrium between cells and the media in which they function. Thus, body fluids reflect the well-being of the community of cells which is the animal, abnormal behavior of the cells being detected by changes from normal in the composition of the body fluids. A study, therefore, of these fluids contributes much to an understanding of the physiological state of the experimental animal. It is toward this understanding that the following review of the composition of the blood and urine of dogs in different physiological states is directed. It is a review of elementary attempts to calibrate the dog for experimental purposes in medical science.

The variations in the volume and composition of the body fluids are relatively large in a heterogeneous group of so-called normal dogs. A normal range of variation is often determined for such groups. However, it is the purpose of this paper to point out that changes within that range may be important in an experiment; that, although large numbers of dogs are often used to get statistically significant results, a few carefully calibrated animals can tell the story more truthfully and accurately. To do this, each dog should be placed in as constant a physiological state as possible, and the range of variation established for that state. Similar physiological states cannot be achieved, however, by putting every animal under the same experimental conditions. The caloric intake of the diet, for example, necessary to maintain a population of dogs in an optimum state of nutrition, can vary, individually, from 40 to 150 calories per day per kilogram of body weight. One dog may require much more protein than another to maintain nitro-

* These studies were supported in part by the Protein Metabolism Fund of the Bureau of Biological Research.

gen equilibrium. Thus, the caloric intake and nitrogen intake must be adjusted for each dog, to bring the population into similar physiological states. Other dietary factors must also be controlled. Hence, a semi-synthetic diet[1] is recommended for carefully controlled experiments.

The plasma volume was found by Gregersen and Stewart[5] to vary between 35 and 65 ml. per kg. of body weight in a heterogeneous group of dogs. However, as these authors point out, individual variation is not nearly so large. Indeed, under controlled conditions, plasma vol-

TABLE 1

PLASMA AND AVAILABLE FLUID VOLUMES IN FOUR DOGS

(Sample 2 was taken 5 days after sample 1. Plasma volumes were determined by using the dye T1824; available fluids, by using Na SCN.)

Dog No.	Sample No.	Plasma volume (P)	Available fluid (A)	A/P
28	1	529	2470	4.7
	2	527	3090	5.9
42	1	500	2700	5.4
	2	509	2640	5.2
65	1	557	2700	4.9
	2	560	3000	5.4
68	1	749	3140	4.2
	2	731	3850	5.3

umes can be kept constant in single individuals within the error of the determination. The data in TABLE 1 illustrate the agreement which may be expected in plasma volumes and "available fluid volumes," in dogs kept under such conditions. One of the changes which will effect plasma volume markedly is a shift in protein stores of the animals. Reduction in plasma albumin, for example, is reflected by a fall in plasma volume, and a rise in available fluid. A complete description of these shifts in fluid balance, which accompany changes in protein stores, will be published elsewhere. It is, however, important to note here that changes in labile protein stores in the dog during an experiment can alter markedly the volume of body fluids.

The volume of red blood cells in a population of laboratory dogs varies considerably, averaging around 45 per cent of the whole blood. Individual volumes, also, can vary from time to time, because of the

pooling of these cells in the spleen. However, under controlled conditions, they are quite constant. The reduction in hemoglobin and red blood cell count, as well as leucocyte count, during barbiturate anesthesia, illustrated in TABLE 2, is an exaggerated example of the pooling of these cells in the dilated spleen. The data in the literature[6] demonstrate that the red blood cells increase in numbers, from a few million per c.mm. of blood in young puppies, to an average of 6.2 million in the adult dog. The average corpuscular hemoglobin is 24×10^{-12} gm. in both young and old dogs. The hemoglobin concentration of the blood, therefore, increases from 9 to 10 gm. per 100 ml. of blood, at 2 months of age, to an average of 15.1 gms. in the adult animal. These are all so-

TABLE 2

AVERAGE DATA OBTAINED ON THREE DOGS GIVEN 48.8 GMS. OF DELVINAL SODIUM PER KGM. OF BODY WEIGHT

(*Data taken from* Allison, Seeley, and Morris,[4] *1944*)

Condition of dogs	Hemoglobin	Red blood cells	White blood cells
	gms./100 ml.	Millions per c. mm.	Thousands per c. mm.
Normal	14.6	6.2	15.9
Deep sleep	12.3	5.3	9.7
Awakening	12.6	5.1	10.7
Recovered	14.3	6.3	16.0

called normal values. It may be, however, that the low red blood cell counts so often recorded for young dogs are due in part to dietary deficiencies. Recently, Nakamura, Morris, and Atkinson,[8] for example, reported red cell counts as low as 1.5 to 2.5 million per cubic millimeter, and hemoglobin as low as 7.5 gm. per 100 ml., in puppies receiving milk from a mother fed an inadequate diet. A quick and complete regeneration of cells and hemoglobin was accomplished by treating the puppies twice a week with liver extracts. Regeneration of this type, however, can take place only in the presence of an adequate intake of the right kind of protein. It has been our experience that regeneration is most rapid if the animals receive, daily, at least 0.4 grams of protein nitrogen per kgm. of body weight, together with a salt mixture containing iron, copper, and manganese, and with adequate vitamin therapy.

The white blood cell count, both total and differential, is one of the most useful tools to determine the condition of the dog. Using the

Schilling method of classification,[6] the polymorphonuclear neutrophils are classified as myocyte, juvenile, stab, and segmented forms. The first two forms, however, are rarely found in the blood of the normal dog. The average white count for the normal adult dog is 11,467 (TABLE 3), just about twice that found in man. There is a tendency for young dogs to have a slightly higher white count, with a greater

TABLE 3

COMPARISON OF THE BLOOD PICTURE OF NORMAL DOGS AND DOGS SUFFERING FROM DISTEMPER

(Data compiled primarily from records kept at the Raritan Hospital for Animals, Stelton, N. J.)*

	2–8 months				9 months and older			
	Normal 35 dogs		Distemper 80 dogs		Normal 31 dogs		Distemper 80 dogs	
	Average Number	Average per cent	Average Number	Average per cent	Average Number	Average per cent	Average Number	Average per cent
W.B.C.	12165	—	12611		11467	—	12123	
Seg.	6795	55.85	8115	64.35	7525	65.27	8451	69.71
Lymph.	4051	33.30	2050	16.26	2491	21.72	1492	12.31
Stab.	816	6.70	2050	16.26	753	6.56	1652	13.63
Eosin.	484	3.97	71	0.56	623	5.43	180	1.48
Mono.	19	0.15	147	1.17	77	0.67	145	1.20

proportion of lymphocytes, and fewer segmented forms than adult dogs, the numbers of lymphocytes being more labile, as they respond more to changing physiological conditions in young, than in adult, dogs.

A decrease from normal in the percentage of lymphocytes, and the increase in percentage of stab cells, are characteristic of dogs suffering from distemper. The average total number of white blood cells recorded in TABLE 3 are not changed from normal in the distemper dogs. Nevertheless, these average figures can be misleading, since, with a pure virus distemper, the total number of white blood cells is always reduced below the normal range. In virus distemper, the number of white blood cells is reduced, accompanied by a reduction in the percentage of lymphocytes and an increase in neutrophils. A dog in this physiological state is very susceptible to secondary infections. These infec-

* Acknowledgement is made to Dr. M. L. Morris of the Raritan Hospital for Animals, for permission to compile these data.

tions usually cause an increase in the total numbers of white blood cells, often increasing them well above normal. Thus, a leucocytosis may accompany distemper, if secondary infections are present, but, in a heterogenous population of distemper dogs, white blood cell counts may range from below, through average, to well above normal. The differential, however, regardless of the white count, will always show the characteristic "shift to the left."

A marked leucopenia and a polymorphonuclear leucocytosis is an unfavorable response to distemper. Dogs in this condition can appear normal to the laboratory worker, but are very susceptible to secondary infections, developing meningo-encephalitis,[1] and other diseases. It is not uncommon for a dog suddenly to sicken and die, after apparently recovering from distemper. Such animals are always in a condition which produces a marked lymphocytic leucopenia. Indeed, a low lymphocyte count in a dog is always associated with lowered resistance to disease, possibly reflecting lowered antibody formation. Certainly, such dogs, when depleted in proteins, are very susceptible to infections. One dog, apparently normal, but with a low lymphocyte count, placed on a protein-free diet, always developed a severe sore throat, which could be cured only by repletion on a good protein diet, such as whole egg.

Depletion in body protein stores is reflected by a reduction in plasma proteins including antibody proteins, reductions in plasma volume, increase in available fluid volume, reduction in excretion of body nitrogen, and a decrease in resistance to disease.[2] Wounds heal very slowly in depleted dogs, sores sometimes called kennel sores developing in their skin. It is not unusual to find pound and even laboratory dogs in this depleted condition. If such dogs are used as experimental animals, they will yield results which are quite different from those obtained on animals with adequate protein stores. The nitrogen balance index, for example, of casein determined in a normal dog is 0.8, while in a protein-depleted animal this index may be as high as 1.0.[3] In general, if the plasma proteins are less than 5.5 grams per 100 ml., the nitrogen balance index will be abnormally high and the excretion of urea nitrogen abnormally low.

Many other examples of biochemical and physiological standardizations of dogs, correlated with physiological states and experimental results, could be given, but would be beyond the bounds of this short

paper. It might be helpful, however, to reemphasize the necessity of a careful study of the experimental variables under controlled conditions. The data in TABLE 4 were obtained on four dogs, during a period of approximately two weeks, while they were being prepared for an experiment which involved, among other variables, the hematocrit, blood urea, and liver function tests. The data in the table show that the per cent red blood cells are normal and quite constant; yet, there may be a tendency, ever so slight, for the per cent cells to decrease over this two

TABLE 4

HEMATOCRIT, BLOOD UREA, AND BROMSULPHALEIN TESTS ON FOUR DOGS DURING A CONTROL PERIOD OF APPROXIMATELY TWO WEEKS

Dog No.	Red Blood cells, %			Blood urea, mgs. %			Bromsulphalein test, %		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
1	46.9	45.4	45.1	16.9	17.6	17.3	2	9	7
2	44.9	45.0	42.3	10.6	10.4	11.2	19	23	15
3	45.9	46.0	44.4	9.3	8.2	8.8	4	10	9
4	49.5	47.5	46.3	8.6	7.4	9.0	8	—	12

week control period. This slight decrease warrants further investigations, especially since it is possible that the vitamin content of the diet and the intestinal flora of the dog may have been deficient in certain hemoglobin-forming factors. The blood urea nitrogen concentrations are all within the normal range for the four dogs, and yet dogs 1 and 2 have higher blood urea nitrogens than dogs 3 and 4. Preliminary studies, which will be amplified elsewhere, indicate that these differences are correlated with the utilization of dietary nitrogen in the presence of varying protein stores of the animal. Finally, dog 2 is separated from the other three by the bromsulphalein test for liver function, this dog having a decreased liver activity toward the dye.

Thus, though variables may be within a so-called normal range, significant differences can be established within that range. Each animal must be considered as a complete instrument of measurement, which records significant results. The interpretation of these is dependent upon knowledge of the state of the instrument at the time the record is made. A few carefully calibrated animals will tell the story more truthfully and accurately than a large heterogeneous group, no matter how clever the statistical treatment of the data from the group may be.

LITERATURE CITED

1. **Allison, J. B., & J. A. Anderson**
1945. J. Nutrition **29**: 413.
2. **Allison, J. B., J. A. Anderson, & R. D. Seeley**
1946. Ann. N. Y. Acad. Sci. **47**: 3. (In press.)
3. **Allison, J. B., R. D. Seeley, J. H. Brown, & J. A. Anderson**
1946. J. Nutrition **31**: 237.
4. **Allison, J. B., R. D. Seeley, & M. L. Morris**
1944. Am. J. Vet. Res. **5**: 62.
5. **Gregersen, M. I., & J. D. Stewart**
1939. Am. J. Physiol. **125**: 142.
6. **Morris, M. L., J. B. Allison, & D. F. Green**
1940. J. Lab. & Clin. Med. **25**: 353.
7. **Morris, M. L., & T. J. Murray**
1940. J. Am. Vet. Med. Assoc. **96**: 80.
8. **Nakamura, F. I., M. L. Morris, & L. N. Atkinson**
1945. J. Am. Vet. Med. Assoc. **106**: 336.

DOCTOR MARK L. MORRIS, New Brunswick, N. J.: *Some Practical Aspects of Selecting and Maintaining Dogs for Medical Research.*

The practical aspects of selecting and maintaining dogs for medical research will be considered under five headings.

The *availability* of suitable dogs constitutes a problem, and the situation is becoming increasingly more difficult. In the past, dogs have been available from three sources, the principal one being the animal dealer. Such a person makes a business of buying, raising, and shipping dogs to institutions or laboratories. The supply from these sources is not too adequate, and the quality of animals obtained is, for the most part, unsatisfactory. The initial cost per dog is reasonable, in fact, very cheap. However, the mortality rate of the purchased stock is high, often reaching 75–100%. If *all* of the expense is assigned against the conditioning of the dog, up until the time the animal is successfully employed for study, then the total cost is much greater.

Dogs are also obtained from pounds and shelters. Such supplies are even more unreliable: numbers available fluctuate widely, and the quality is usually unsatisfactory. The mortality rate of animals from pounds is perhaps even higher than of those obtained from animal dealers, because most dogs that find their way into pounds are usually susceptible to infections commonly inhabiting the premises. The initial cost is cheap; but, again, the end cost per animal successfully used is quite high, perhaps even higher, than of those obtained from animal dealers. When dogs are purchased from wardens in charge of pounds, such a practice encourages the collecting of dogs from the street. This may be a desirable practice; but the average pet animal owner permits his dog to use the street as an exercising yard, and, consequently, these pet animals are frequently impounded and subsequently reach a research laboratory. If the requirement of laboratories is active and the supply is short, in an effort to meet the demand it is not uncommon for the unscrupulous warden or his employee to remove and destroy collars bearing identifying licenses. It is unnecessary to point out that such a practice creates animosity in the minds of the public, and they turn for relief to the various societies organized for the purpose of protecting the welfare of animals. These organizations have influen-

tial public relations contacts, so that appeals can be made personally, or through the press or the radio. In an effort to assist the complaining public, the societies seize upon the opportunity to be of service, making a target of research laboratories and their personnel. A serious public relations problem is created, and the laboratory is forced into a defensive position. If the animals originated as the private property of the laboratory, there would be less criticism from animal welfare societies.

The supply of dogs from privately owned kennels is, at present, very limited. Owners and breeders are usually not interested in raising dogs for medical research, but breed the animals for show purposes, or to be sold as pets. In fact, many would seriously object to selling dogs to laboratories, if the animals were to be used for medical research. The present and future supply of dogs from privately owned kennels may, for the most part, be largely discounted. It can, thus, be stated that the majority of animals at present employed in laboratories are obtained either from animal dealers, or from pounds and shelters.

The *housing* of animals used by laboratories may be considered under three headings: general physical facilities, individual kennels, and exercising yards. The general physical facilities, from what I have observed, vary widely, the most common being cellars, attics, and back rooms. These animal houses often have no light, poor ventilation, and unsanitary conditions. In contrast, there are some laboratories which have modern facilities, providing light and ventilation, and the construction is such that floors and walls can be washed and kept sanitary. The individual kennels vary from either single cages or metabolism units to wire enclosures for confining groups of from five to twenty dogs. The large enclosure may be practical, but it encourages fighting, breeding, dissemination of disease, and poor feeding habits. Under these conditions, the outcome is frequently a survival of the fittest, the larger and stronger dog gulping the food, the smaller and less fortunate animal surviving on left-overs.

In most instances, yards or facilities for exercising are not provided. If present, these units are usually small indoor enclosures, either with or without drainage, constructed to accommodate 20 or more animals, and thus encouraging a high concentration of excreta in a small area. Outdoor exercising yards with fresh air, natural light, and good drainage, are essentially non-existent. Visitors to research laboratories cannot help but feel sympathetic toward creatures con-

fined under crowded unsanitary conditions. In an effort to improve the care and handling of the dogs, an employee or staff member is often instrumental in offering suggestions to qualified persons. This encourages an investigation, and develops adverse publicity for the laboratory, the personnel, and for the practice of using dogs for medical research.

The *feeding* of dogs used by laboratories is very important. This problem may be considered under two headings: the quality of the food, and the method of feeding, both prior to purchase and during the animal's stay in the colony. The institution purchasing the dog has no control over the quality of food or method of feeding. However, each has an important bearing on the condition of the dog, at the time of purchase, and for some time subsequent thereto. When the animal is added to the colony, its nutrition varies widely, depending upon the food available and the purpose for which the dog is to be used. Usually, the ration supplied by the institution is of unknown biological quality, having been purchased from a producer who is a good customer, client, or friend of the company, or because the particular ration is one employed by some other well-known laboratory. The palatability and nutritional quality are too frequently secondary, so long as the food is reasonable in price and the animals consume the ration and continue to live.

Disease control is obviously fundamental, and must be given consideration, both prior to purchase and during membership of the colony. Control of disease in dogs requires time, skill, and expensive materials. Animal dealers, dog wardens, or pound keepers are not interested, and the housing and feeding techniques employed by such persons encourage the development of disease. It is difficult to obtain a dog from a pound that has not been exposed to, or is incubating, a parasitic, bacterial, or virus-induced disease. The desire of the animal dealer is to turn the dogs over as rapidly as possible, and collect the fee before the animal becomes ill and dies. Towards an addition to the colony, some attempt at control is made by most institutions. However, the additions continue to supply ample quantities of viruses, bacteria, and parasites to nutritionally deficient stock. Old resident members of the colony are usually immune to "everything in the book."

Experimental animals are fed, prior to purchase, all sorts of rations of unknown values, while those maintained in laboratory colonies

are fed rather empirically. Disease control is on a somewhat comparable basis, little effort being made by the animal dealer. Most institutions have inadequate isolation and housing facilities, plus an insufficient number of qualified trained personnel to cope with the problem. As a result of malnutrition and disease, many dogs used for research purposes suffer from varying degrees of organic damage, chiefly of the kidney and liver. These functional changes directly affect the biological status of the animal, so that the results of many physiological, pathological, and pharmacological investigations are adversely influenced. Thus, it can be stated that the average dog used for medical research is a malnourished, diseased, pathological specimen, often improperly housed and nourished, but used to obtain physiological and pharmacological data. In selecting dogs for various projects, the physiological state is not taken too seriously, the investigator's criterion being frequently: "That one looks okay; we'll use him." Subsequently, however, the animal proves unsatisfactory, fails on the project, and is discarded. The cost of the dog, preliminary preparation, and the investigators' time and materials are a total loss. If the investigation is completed, one is never certain of the results; attempts to repeat the study on the same animal or one of the same species fail to produce comparable data. It is surprising that more employees are not bitten by rabid animals, since it is the function of the dog warden to remove stray animals from the streets. These campaigns are vigorously carried on when rabies is most prevalent. Leptospirosis and certain fungus infections may also be contracted from diseased dogs, and these conditions are frequent among pound animals. Certainly, some hazard to employees must exist.

A critical analysis of the problem is essential, if a solution is expected. Is there any approach, or can any recommendations be made, which would assist in improving the biological status of dogs now widely employed in medical research? If laboratories wish to continue the present practice of purchasing animals from dealers, pounds, or shelters, then some system for screening should be devised which will immediately eliminate the most evident pathological specimens. It will be almost impossible to obtain diet histories on animals procured from dealers or pounds. Therefore, some plan should be devised for rehabilitating the malnourished and diseased. These procedures should be undertaken in quarters entirely separate from the

main colony. Disease should, in so far as possible, be eradicated, and proper immunization applied.

In addition to improving present methods, it would seem that some thought might well be given to the development of a so-called standard-bred animal of known genetic background, free of parasitic or infectious diseases, properly immunized, and fed a balanced, nutritionally complete diet. Such an animal should be of convenient size to be tractable. It should have good veins and arteries, understanding, and a tolerant disposition. Temperamental, neurotic creatures should be avoided. A good, rugged, disease-resistant strain, possibly developed by cross-breeding certain pure strains, might prove ideal. It is well known that the dog is the most valuable animal employed in medical research for obtaining reliable data applicable to the human. To accomplish these purposes, a properly organized, concentrated effort must be made.

On several occasions, representatives of institutions and industries have discussed this problem with us. It would seem that a closely-knit program, sponsored by industrial and institutional laboratories, is needed to develop, finance, and administer a program which would make dogs of uniform size, scientifically fed, and free of disease, available for medical research. Millions of dollars are expended annually on various types of instruments and mechanical equipment. Is it not logical that a comparable effort should be made to improve the quality, feeding, housing, and methods of handling animals which make a major contribution to medical research and human welfare?

Appreciation is expressed to my colleagues and friends who offered comments and criticisms.

SECTION OF PSYCHOLOGY

MAY 20, 1946

DOCTOR EDMUND JACOBSON, Laboratory for Clinical Physiology, Chicago, Illinois: *"Electrical Measurements of Mental Activities in Man.* (This lecture was illustrated by lantern slides.)

The view that mental activities, such as imagination, perception, recollection, and emotion, occur in and through the brain alone has been almost universal for centuries. During a series of observations of sensory experiences which I began in 1908, and in which I later had the assistance of graduate students and faculty members whom I trained especially for the purpose, I was led to doubt the accuracy of the traditional view. I believed that I could detect sensory and motor experiences, evident, not in the brain, but in skeletal and smooth muscles and elsewhere, which seemed indistinguishable from the conscious processes present during the mental activity under observation, and which seemed, to all intents and purposes, to constitute all or part of the mental activity itself. However, these subjective observations, no matter what the controls, failed in themselves to constitute proof that mental activity is not confined as a function to closed circuits within the brain. Objective tests, based upon the subjective findings, were obviously the next step.

Accordingly, after many years of effort, electrical apparatus of unique sensitivity was devised and set up in 1927, capable of measuring transient voltages as small as one millionth of a volt, under special conditions and limitations. Suitable electrodes were devised, and placed in contact with skin overlying muscles to be tested. In later studies, wire electrodes were inserted into the muscles or into the superficial nerves which were under investigation.

During the mental activity studied, which generally lasted about a second or more, action-potentials were discovered in the muscle or muscles which would naturally participate if the actual act were to be carried out, but in an abbreviated and fainter fashion. For example, if the subject imagined that he was lifting a ten-pound weight with his right hand, action-potentials would arise promptly after the recorded

signal to imagine and would cease promptly after the recorded signal to discontinue to imagine. However, this did not occur uniformly in all subjects, for some reported that they merely visualized themselves lifting the weight. In these, the electrical records generally indicated merely eye-movement to imagine. Other subjects showed electrical phenomena, as described, simultaneously in the eye and arm regions.

With the aid of sensitive mechanical contrivances, it was shown that the action-potentials recorded in electrodes connected with a muscle were accompanied by actual, but minute, contractions in that muscle. In the instance where the subject was instructed to imagine lifting a ten-pound weight with his right hand, the muscles on the right side actually contracted minutely, as if to lift that weight. But in relaxed subjects, the muscles on the side showed no electrical or mechanical signs of contraction.

Mental activities referring to abstract matters were commonly carried out in terms of eye or speech organ activities which could be electrically recorded and identified.

Since these earlier investigations, many subjects have been trained to relax, as is essential for the many types of control tests necessary in these investigations. When the subject relaxes his skeletal musculature to a very advanced degree during a certain interval, he later reports that mental activity was diminished or absent for the time. Corresponding with the subject's report, action-potentials are generally diminished or absent in the specific region under test.

Evidence has been found in this laboratory that individuals, both healthy and diseased, can be trained to relax. Individual differences obtain, but a general trend is manifested in electrical recordings in most instances. Training effects an increased ability in the individual to control his mental activities in the direction of relaxation. This can have practical applications.

SECTION OF ANTHROPOLOGY

MAY 27, 1946

DOCTOR RUTH BENEDICT, Associate Professor of Anthropology, Columbia University, New York, N. Y.: *The Study of Cultural Patterns in European Nations.*

Every nation in Europe and Asia has simultaneously denied and boasted that it had a national character. It has been almost impossible to separate the wheat from the chaff in the extravagant statements that have been put forth, and many social scientists have been inclined to chalk up the whole problem as a subject for popular oratory, and to throw it out of court as a matter for systematic investigation. During the war years, however, the problem of national character became a matter of grave practical importance. There were crucial questions as to "the nature of the enemy," the receptivity of satellite nations to certain kinds of appeals and not to others, and the opposition of certain of the allied nations to measures easily accepted by others. Those who were engaged in psychological warfare, in political conferences, in the training of OSS and UNRRA personnel, as well as those engaged in military operations, were constantly handicapped if they made mistakes in estimating the way a *vis-à-vis* nation would think and behave. It was of the utmost importance to eliminate popular fantasies and misapprehensions, and to use whatever techniques the social sciences could offer, in order to understand these national characters.

To the anthropologist, the study of national character is a study of learned cultural behavior. For several decades before the war, anthropologists had done pioneer work, in this field, in compact primitive communities. During the last decade, theoretical points made by anthropologists about cultural conditioning had been widely accepted. Anthropologists had presented their case convincingly enough so that there was wide agreement that social arrangements are of fundamental importance in shaping any people's tenets about life, whether they are assumptions about the function of the State, economic motivations, relations between the sexes, or dependence upon the supernatural. The

forms these tenets take in our own cultural background were no longer generally considered to be direct consequences of human biology, and "human nature" was no longer considered as a sufficient explanation of them. Behavior, even in civilized nations, was increasingly understood as ways of acting and thinking which developed in the special kind of social environment characteristic of that part of the world.

In 1943, I was asked to join the Office of War Information, to work on national character in enemy and occupied countries. I was asked to use the insights and techniques anthropology had developed in the study of learned cultural behavior. In spite of all the necessary limitations imposed upon research in the social sciences during war, it was a great opportunity. Studies of modern society had very seldom, indeed, made systematic use of the methods upon which anthropologists had based their analyses of the simpler societies. These methods were quite specific, and were designed to investigate how each new generation had learned and transmitted its way of life in all its specificities. They were methods for detailed studies of specific social environments. Experience had shown that it was necessary to stress many aspects of life which rate as trivia in Western international studies. Habit formation in a specific social environment; the rewards and punishments bestowed by society; the praise allotted to certain kinds of achievement; the connotations given to exercise of authority, and to submission to it, in day-by-day living; the degree to which responsibility for his own conduct was entrusted to the individual—all such questions had been regarded as essential in cultural investigations of behavior in primitive societies, and had hardly been raised in studies of European nations. In classic studies of civilized countries, the approach is, ordinarily, either historical, or economic, or political. Though such segmented approaches are valuable and necessary, they still leave a wide field for applications of methods which have been successfully used in anthropological studies of learned cultural behavior.

In attempting, during the war, to use such techniques in the study of civilized nations, there was a grave handicap, occasioned not by the nature of the research, but by the fact of war. The anthropologist's chief technique, that of the field trip, was impossible. There were available, however, in the United States, persons of almost every nation of the world, and it was a fairly simple matter to find transplanted groups which retained a great deal of the way of life to which

the older members had been born. Individuals could be found from most classes and minorities, and from most of the distinctive provinces of a nation. It was not necessary to give up the traditional anthropological reliance upon face-to-face study, and this recourse to informants was all the more necessary, the clearer it became that much essential material for the studies I had been asked to make was not elsewhere available.

The usual comment on such projected studies of civilized countries is that, quite apart from the limitations imposed by the war, civilized nations are too difficult to study by methods that may be sufficient in small communities. Such skepticism is often based on what should rather be regarded as a great advantage: the multiplicity of the facts known and recorded about Western nations. Actually, the anthropologist working on civilized nations has a great head-start, in that much work has been done in historical research; that statistics are available in many fields; that so many observers have recorded their personal experiences; that there are often excellent novels available; and that the language does not present the grave obstacles it does in tribes where it has never been recorded and ordered in grammatical categories. Vast quantities of material are a handicap only when the crucial problems to be investigated are not formulated. When they are, it is possible to cull the relevant material from the most diverse sources. The richness of the data is an asset, and, when lacunae were discovered, it was usually possible to obtain necessary facts from informants. The principal advantage the anthropologist had, was that certain ways of stating the problems had emerged from his experience, and stating the problem so that it can be answered by research is usually half the battle.

Skepticism about the application of anthropological techniques to civilized nations is also often based on the lack of cultural homogeneity in modern nations. This kind of skepticism frequently seems to the anthropologist to be no criticism of his method, but a statement of an elementary principle which he completely accepts. No anthropologist, I think, would attempt to study "the" character structure of such a welter of cultures as were included within the national boundaries of Jugoslavia. There are other multicultural states such as Poland and Czechoslovakia. Such conditions do not mean that investigation must be abandoned. The solution is to multiply the number of investiga-

tions to any desired point, and this holds true also of such lesser problems as are presented by the different regions of England, or of France, or of the United States.

The criticism that the degree of class differentiation prevalent in Western civilization makes the use of anthropological methods impossible, stands on a somewhat different footing. Adequate cultural study of this situation, including all the relevant factors, has hardly been attempted in Western nations. Such a study would investigate what attitudes and convictions the various classes have in common in any nation, as well as the obvious fact of conflict of interests. Even the conflict situation is usually inadequately stated. The trained anthropologist, in any study of complementary behavior, whether between authoritarian fathers and submissive sons, or between despotic kings and their subjects, has to present both parties as actors in a patterned situation. He can see it as a kind of see-saw, and by studying the height of the fulcrum and the length of the board (in the study of classes, laws about property and land, general conditions of social security, and the like), he can show either that the group on the high end of the see-saw is necessarily very far up and the group on the low end very far down, or that they are more nearly balanced. As extremes, material or psychological, are eliminated from one position, extremes will also be eliminated from the other. Other groups in the society, too, may throw their weight now to one party and now to the other. The anthropologist has good reason to know, also, that nonmaterial factors may be as important as material ones in any given situation, and he investigates, for instance, the cultural acceptance of hierarchy, as well as the relative frequency of wealth and poverty.

The similarity of the basic assumptions about life made by both those classes, in any nation, is of great importance. The wealthy industrialist and the laborer or peasant, in a nation or area of Western civilization, hold many attitudes in common. The attitude toward property only in part depends upon whether one is rich or whether one is poor. Property may be, as in Holland, something which is an almost inseparable part of one's own self-esteem, something to be added to, kept immaculately, and never spent carelessly. This is true, whether the individual belongs to court circles or can only say in the words of a proverbial expression: "If it's only a penny a year, lay it by." Alternatively, the attitude toward property may be quite dif-

ferent, as in Roumania. An upper-class person may be, or become, a pensioner of a wealthy man, without loss of status or self-confidence; his property, he says, is not "himself." And the poor peasant argues that, being poor, it is futile for him to lay anything by; "he would," he says, "if he were rich." The well-to-do increase their possessions by other means than thrift, and the traditional attitude toward property differences associates wealth with luck or exploitation, rather than with assured position as in Holland. In each of these countries, as in other European nations, many of which have deeply embedded special attitudes toward property, the specific nature of these assumptions can be greatly clarified by study of what is required of the child in his handling and ownership of property, and under what sanctions and conditions expanding opportunities are allowed in adolescence, and at his induction into fully adult status.

Attitudes toward authority are similarly localized. A Greek, whether he belongs to the upper classes, or whether he is a peasant villager, has a characteristic opposition to authority from above, which permeates daily conversation and influences his choice of a means of livelihood quite as much as it colors his political attitudes. On the other hand, it is quite true that, in other regions of Europe, in the dramatic words of Ortega y Gasset, there has been a "formidable cry rising like the howling of innumerable dogs asking for someone or something to take command, to impose an occupation, a duty." During the war, Goebbels's propaganda broadcasts quoted the well-known words of Machiavelli, saying that all Germans knew they were true: "Men work either under compulsion, or of their own will. The greatest energy they display where their own choice has the least freedom." Such authoritarianism deserves the closest cultural study. It requires knowledge, not only of the laws and of the economic set-up that have fostered it, but of the child's first experiences with authority, and of the sanctions which are invoked. It requires knowledge of the age at which various disciplines are imposed, and of the rewards of obedience. Such knowledge can lead directly to a clearer insight into what the leaders in any country are saying in their political speeches, and into what courses of action the people of that country can advantageously follow in reconstruction. Character structure can, of course, change over generations, as different experiences are provided, but the very process of change can be illuminated by systematic study of behavior in this generation.

At present, even the recorded facts necessary for a cultural study of the nations of Europe are widely scattered in different publications, and much crucial information is not recorded at all. Our understanding of international affairs is about where our understanding of primitive peoples was before the anthropologists attempted the serious study of how primitive people *learned* their cultural behavior. Even those students who have used the method in the simpler cultures have usually laid it aside when they came to discussions of our own civilization. They tend to assume a similarity in experience among the different Western nations, which my investigations showed did not exist. There are differences of the same order as those with which we are familiar in the isolated simpler cultures. We need studies of Western peoples which show them to us as people who have learned, in specific ways, to solve the universal human problems by special cultural arrangements to which they give their allegiance as we do to ours. We need intimate understanding of their experiences, so that we shall learn to discriminate between what is truly socially dangerous and what is only another method of arriving at a socially desirable goal. The kinds of strength which the people of each area could use in a world organized for peace can only be those to which they have been bred. If we insist that they imitate another kind of strength, they will be powerless to contribute. If we, the people of the world, are ever to achieve a world organization which promises mutual benefits, we must be scientifically prepared to know the strength which different nations of the world can utilize to this end.

SECTION OF OCEANOGRAPHY AND METEOROLOGY

MAY 24 AND 25, 1946

Conference on *"Convection Patterns in the Atmosphere and Ocean."*

The Section of Oceanography and Meteorology held a Conference on "Convection Patterns in the Atmosphere and Ocean." Doctor Athelstan F. Spilhaus, New York University, College of Engineering, New York, N. Y., was the Conference Chairman, in charge of the meeting.

The program consisted of the following papers:

FRIDAY, MAY 24

Morning Session. Chairman, Athelstan F. Spilhaus.

"Conference Introduction, Problems Concerning Convective Layers," by R. B. Montgomery, Woods Hole Oceanographic Institution, Woods Hole, Massachusetts.

"A Summary of the Theory of Convection Cells," by Henry Stommel, Woods Hole Oceanographic Institution, Woods Hole, Massachusetts.

"Internal Waves in the Atmosphere and Convection Patterns," by B. Haurwitz, Massachusetts Institute of Technology, Cambridge, Massachusetts.

Afternoon Session. Chairman, J. Bjerknes, University of California, Los Angeles, California.

"Convection of Aerosols," by Worth H. Rodebush, University of Illinois, Urbana, Illinois.

"Convective Motion in Air over the Sea," by A. H. Woodcock and Jeffries Wyman, Woods Hole Oceanographic Institution, Woods Hole, Massachusetts.

"Structure of Summer Rain as Detected by Radar," by Raymond Wexler, Evans Signal Laboratory, Belmar, New Jersey.

"Observations of Vertical Temperature and Humidity Distributions in the Convective Layer above the Sea Surface," by Richard A.

Craig, Woods Hole Oceanographic Institution, Woods Hole, Massachusetts.

SATURDAY, MAY 25

Chairman, H. U. Sverdrup, University of California, The Scripps Institution of Oceanography, La Jolla, California.

"Convection in the Annual Temperature Cycle of Lake Michigan," by Phil E. Church, University of Washington, Seattle, Washington.

"The Maintenance of Instability in the Surface Waters of the Ocean," by John C. Armstrong, American Museum of Natural History, New York, N. Y.

SECTIONS OF BIOLOGY AND PHYSICS AND CHEMISTRY

MAY 29, 1946

Conference on *"Folic Acid."*

The Sections of Biology, and Physics and Chemistry held a Conference on "Folic Acid." Doctor E. H. Northey, American Cyanamid Company, Stamford, Connecticut, was the Conference Chairman, in charge of the meeting.

The program consisted of the following papers:

"Historical Introduction," by W. H. Peterson, University of Wisconsin, Madison, Wisconsin.

"Isolation of Liver *L. Casei* Factor," by E. L. R. Stokstad and collaborators, Lederle Laboratories, Pearl River, N. Y.

"Isolation of Fermentation *L. Casei* Factor," by Brian Hutchings and collaborators, Lederle Laboratories, Pearl River, N. Y.

"Degradation of *L. Casei* Factor by Alkaline Hydrolysis," by E. L. R. Stokstad and collaborators, Lederle Laboratories, Pearl River, N. Y.

"Degradation of *L. Casei* Factor by Sulfite Cleavage," by Brian Hutchings and collaborators, Lederle Laboratories, Pearl River, N. Y.

"Structure of *L. Casei* Factor," by J. H. Mowat and collaborators, Lederle Laboratories, Pearl River, N. Y.

"Synthesis of Pteroic Acid and its Derivatives":

First paper, by Coy Waller and collaborators, Lederle Laboratories, Pearl River, N. Y.

Second paper, by M. E. Hultquist and collaborators, Calco Chemical Company, Bound Brook, N. J.

"Pharmacological Studies," by B. K. Harned and collaborators, Lederle Laboratories, Pearl River, N. Y.

"Physiological Aspects," by F. S. Daft, National Institute of Health, Bethesda, Maryland.

"Vitamin M Deficiency," by John R. Totter, University of Arkansas, School of Medicine, Fayetteville, Arkansas.

"Clinical Aspects," by T. D. Spies, Hillman Hospital, Birmingham, Alabama.